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THE JOURNAL OF PHILOSOPHY

THE SYNOPTIC METHOD

SINCE Plato, in the *Republic,* said that "the dialectical mind is the synoptical," it has been customary for philosophers of various schools to say that it is their task to interpret reality synoptically. The dialectical ideal for philosophical conceptions is that they should form an organic whole, and English philosophers since Martineau [1] have spoken of this demand for unity as the synoptic aspect of their thought. In contrast to this dialectical ideal, the special sciences have generally been said not to be synoptic, but abstract. And in so far as synopsis is the method of ordering all the categories of being in one system, philosophy alone deserves to be called synoptical.

But the word has of late taken on new meanings, and it is commonplace today to say that the sciences are synoptical, in that they investigate their objects as wholes, supplement the analytical inquiries with a regard to the unique properties of their objects which are said to be lost or at least neglected in many forms of analysis. In biology, organismic theories are replacing narrow mechanistic and vitalistic conceptions which seek to explain the unity of the organism as a result of certain conditions of its parts; psychology has largely abandoned atomism; even in physics the procedure which would assert the sole adequate account of molar phenomena to be in terms of microscopic products of analysis has apparently shown difficulties.

That these new approaches in science may legitimately be called synoptic, even though they are not comprehensive like philosophy, may be seen from a comparison of the types of objects which are investigated in metaphysics and in the special sciences. The philosopher demands systematic unity in his conceptions simply because he believes the world, at least in so far as it is rational at all, to be a unity, a whole. It may have complexities and irreducible differences, conflicts, and contradictions in it, but after all he supposes it to be one world even though in some senses it may be a plurality: since Plato wrote his *Parmenides,* there has been no difficulty in supposing it to be both one and many. Like-

[1] James Martineau, "The Unity of Mind in Nature" (1853). (In *Essays, Reviews and Addresses,* London, 1891, Vol. III, p. 105.

wise, the scientist who is earning the name "synoptist" deals with his object as a whole: it would be only a monistic prejudice which would deny that some things, though parts of the world to be sure, may be wholes without being *the* whole. And the psychologist or biologist in supposing the personality or the organism to be a whole requires a consistency of assertions concerning it which is quite analogous to that which the metaphysician requires when he makes assertions about the whole world.

It is the nature of the method of dealing with such wholes which I wish to investigate. To do this requires a consideration of our experience of wholes and of the criteria to which we appeal when we say a thing is a whole. The former inquiry will lead us to the striking conclusion that those who have offered definitions of synopsis have not by any means meant what they said.

Obviously, a definition of synopsis must involve some reference to a whole. Etymologically, the word means "seeing together," and a "togetherness" of a set of objects is at least one condition for their being parts of a whole. But "seeing together" applies to two different procedures: a process of seeing things in their togetherness or a process of seeing together various things. As an example of the first, the word "synopsis" has been used to refer to the experience of the continuum of immediacy, for it is a complex unity of experience of a variety of content which is not synthesized from parts, but which can be analyzed and broken up into "elements." Kant uses the word "synopsis" in this way; he says, "As sense contains a manifold in its intuition, I ascribe to it a synopsis."[2] But since the primary reference here is to the wholeness of an awareness rather than to the knowing of a whole, we obviously have a situation which is altogether universal and necessary for knowledge in general. Synopsis, as a word used by the layman, means an extension of this comprehension, a syllabus or survey. But this is not what we are here investigating, a controversial method in science. I do not deny, of course, that an extension and clarification of this comprehension is an essential part of all fruitful thinking, and the importance of the original synopsis for methodology should not be neglected. It appears as the first extended apprehension, the *prius* of the analysis which is to follow. Subsequent analysis has it as material and as the guide to the synthesis of the analytical results in a clearer, more articulated, presentation of the original subject-matter.

The first serious attempt to describe the seeing of things together was made by J. Theodore Merz, who used "synopsis" as a translation of *vue d'ensemble*. His description of synopsis as the

[2] *Critique of Pure Reason*, A 97.

view to which "every object of contemplation . . . is a whole, a totality,"[3] has been repeated and accepted without question by those who urge the synoptic methods in philosophy and the sciences. But fundamentally, this definition and the others like it are not descriptive of the procedures which do go under the name of synopsis. For it is elementarism—the theory that the properties of wholes are inherent in the parts—which in effect regards every object as a whole. To take an example, the one-sided development of biology following Schwann resulted from the exclusive interest in accounting for the whole organism as a resultant of the parts, i.e., its cells. But this had as its corollary the centering of interest in the cell itself as in no essential sense a part, i.e., as a whole. True, it occurred in a whole, but it was not significantly different from what it would be in a culture dish where it would be an independent whole. Schwann, but not his followers, recognized that the *organism* as a whole was the seat of some functions (i.e., growth), but it was not until the cell was seen to be not an autonomous whole, but rather a dependent part of a more complex but fundamental whole, the organism, that biology could be said and is said to have become "synoptical." It is then clear that synopsis is not the method which sees all things merely or primarily as wholes.[4]

In dealing with objects in a "togetherness," that togetherness may be merely subjective, as it is when I see a butterfly and a locomotive in one experience-moment; but it may be real and objective, as it is when I know the center and the circumference of a locomotive wheel to be there together, regardless of the fragmentary state of my momentary experience. In the latter case, each object I contemplate is not essentially a whole, but a part. Against Merz's definition, it may be urged that synopsis is a knowledge of an object as a part in its relation to some objective whole.

An important consideration will then be, of course, what the conditions are under which I can assert the object I contemplate to be a part of a whole other than the whole of my experience, of which, of course, every object experienced is a part. This question of the criterion of wholeness and partiality was not clearly put by Merz, but at least the beginning of an attempt to answer it is to be found in another part of his description of synopsis. He believed that synopsis affords more than analysis could ever discover

[3] J. T. Merz, *A History of European Thought in the Nineteenth Century* (4th. ed., London and Edinburgh, 1928), Vol. III, p. 612.

[4] It is interesting to observe that Merz, in accordance with his definition, praised the theory of Gemmules as a "synoptic" theory.

or deal with.[5] But if we inquire into the nature of the alleged inadequacy of analysis it appears that he did not distinguish between an acquaintance and a synopsis. In other words, he failed to see the formal identity of the concept of emergent and that of a Gestalt quality. In making synopsis a knowledge of wholes, he erroneously supposed that its distinctive contribution was an acknowledgment of an oversummative property. Merz's attack on the adequacy of analysis presupposed that the wholes had oversummative properties, and the reference to them was apparently for him a guarantee that he was dealing with a whole and that his method was synoptic, for in analysis he held that "something is lost."

But it is only an accident (of size) that we call some properties "emergents" (e.g., the wetness of water) and others "Gestalt qualities" (e.g., the beauty of a picture). In the former case we do not see the parts of the whole which has the oversummative property, and we call the knowledge of it an acquaintance, a relatively unstructured intuition. In the second, we do see the whole which has the oversummative property; we see its internal structure, and we see that the new property is not a sum of the others. But it does not follow that our experience of *it* is synoptic. Our knowledge of *it*, in its ineffable novelty, is still an acquaintance. The Gestalt property does not have as *its* parts the parts of the Gestalt to which it appertains; synopsis can apprehend this property only because "opsis" or acquaintance in this case is a part of synopsis.

From this we may learn two lessons about synoptic method. The limitation of synopsis to the experience of some "poetic" or incommunicable quality or value of a whole is unfruitful. We have such experiences in cases we do not call synoptic (e.g., any sensuous awareness); and in the cases which we do call synoptic experiences, we do not find the presence of an intuition to be essential to its being a knowledge of structural relationships of parts and wholes. The second lesson is a corollary to this: the presence of an oversummative property, while perhaps a necessary condition of there being a whole, is not a sufficient condition for asserting knowledge of a complex possessing such a quality to be synoptic.

If we consider the structure of wholes, though, it will be possible to discover the desired criterion. The concept "whole" is probably a primitive idea admitting of no definition. But it is possible to exhibit what we mean by the word. When we call an

[5] J. T. Merz, "On the Synoptic Aspect of Reality," *Proc. Durham Philos. Soc.*, 1913, Vol. V, p. 54.

object a whole, we mean at least that it is both singular and plural. It is a single thing, but it is not simple. It is several things considered as one, or one thing considered as several because several things may be found "in it." Since Hume, wholes have been thought of as fictions: cabbages and kings, the counties in England, and myself are three wholes when taken separately, or one whole when taken together, because my assertions have united them in one complex reference. Needless to say, this theory has as disturbing consequences for a theory of wholes as Hume's other opinions had for other parts of metaphysics. Is there no ground for supposing that the head and heart of George III belong in one whole in a sense in which his head and a cabbage do *not* constitute a significant whole?

The most widespread answer to this question would be one which, if true, would limit synopsis to the uses Merz described and which I rejected on the basis of the preceding clarification of his confusion between acquaintance with the property of a whole and synopsis of the objects having the property. This answer asserts that some objects constitute wholes because they give rise to unique qualities which attach to them together but not separately. These properties are oversummative properties, and their presence is a necessary condition of wholeness. Now to show in a more rigorous fashion that synopsis can not be concerned only with wholes, if such a property is the necessary and sufficient condition of the existence of the whole, as it is asserted to be in this answer, we may adopt an argument of Peirce's.[6] Peirce showed that any two objects taken at random possessed a property in common when taken together which neither possessed alone, and which no other object had in common with them. Consider two objects, *A* and *B*. *A* has the property of *a*-ness, and *B* that of *b*-ness. All objects other than *A* have the property of *a*-lessness, and all except *B* have that of *b*-lessness. These two properties constitute a complex property of *a*-*b*-lessness which all objects except *A* and *B* have. Therefore only *A* and *B* possess the property of un-*a*-*b*-lessness when taken together.

This is not by any means trivial. The complex quality of random objects together has the same logical status as the Gestalt quality of a particular type of foolishness attaching to a whole consisting of George III's head and my cabbages, or that unique being of George III enjoyed by all of his organs, or the beauty attaching to a set of esthetically indifferent lines when they are put together.

[6] C. S. Peirce, *Chance, Love, and Logic* (London, 1923), pp. 112–113.

The criterion of individuality which is used in many branches of science is a mere adaptation of this principle of unique qualities. Their discovery seems to be sufficient indication of a real whole, whose parts and structure must be studied in synopsis. The principle of individuality is not narrowly qualitative, of course, but involves unique properties of groups. This can be seen through the extension of Peirce's argument concerning unique quality. The criterion of individuality which is derived from this may be called the Principle of Inhomogeneity. If there are three objects, *a*, *b*, and *c*, such that the relation of *a* to *b* is of a different *kind* from that of *a* to *c* and of *b* to *c*, then *a* and *b* together are inhomogeneous with their context or environment *c* and constitute a whole. For example, the "mechanical individual" is determined by a "balance of motion and rest" among its parts, as Spinoza held; but Spinoza also saw that the relations of motion and rest were so ubiquitous that, strictly speaking, only the whole universe was a mechanical individual. We distinguish some relations, though, between some parts of the mechanical world and other things, e.g., our own purposes. An automobile, though not a mechanical individual since relations among its parts and between it and its context are homogeneously mechanical, is an individual in that a group of objects (its "parts") has a definite and unique relation to our purposes which other objects, such as the roadbed, do not have. These objects, then, are "parts" of a whole, which in this case is determined by a relation to our purposes which, in effect, give some objects (its "parts") when taken together a property which some other things, connected to them by mechanical relations, do not have.

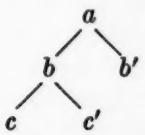
When a procedure of analyzing a manifold of objects reaches certain points, the methods of analysis and the categories on which it is based may have to be changed. I may analyze a body of water, but at one stage I come to an object which can not be treated under categories of hydrodynamics. A fish requires new categories —it is inhomogeneous with its surroundings, and so it is a whole. Not every case is as simple as this; many controversies in biology rest on the diverse answers to the question, When does the investigation require us to introduce specifically biological concepts into the analysis of a situation? This is, in other words, the question, When do we reach a biological whole? What are the limits of the organism?

That this criterion is used there can be no doubt. But it can not be considered faultless. First, inhomogeneity is a relative term, ranging in meaning from Peirce's un-*a*-*b*-lessness on one extreme, which in itself we do not consider empirically significant,

to an organism in an unfavorable environment on the other. There is no adequate means of distinguishing *kinds* of relations from each other; in fact, *kind* itself presupposes significant inhomogeneity. And in the second place, an inhomogeneity might be resolved into a complex homogeneity with more refined methods; this has been the fate of many chemical "elements." The monism of science is a healthy ideal, for this criterion of individuality is a statement of ignorance; that is an individual whole which we have not *yet* reduced to the status of a homogeneous part. We can not attribute individuality at any level merely on this criterion without breaking into the continuity of investigation. Thirdly, when the determination of significant inhomogeneities is intentionally instrumental, as in the case of the automobile, it is *a fortiori* subject to change. For these reasons, this criterion can not be said to give us a dependable guide to the attribution of real wholeness in the object: in every case the admission of inhomogeneities may rest only on our limited experience of the object.

The more dependable criterion of the reality of the whole is to be found in the synoptic consideration of an object which we may suppose for the time being to be a part. If it is a part of an object which is a real whole, it can be exhaustively explained only if in addition to its own intrinsic nature we study also its histrionic nature as playing a rôle in a determinable whole. The synopsis of the object begins with it and examines its relations to its parts and then its relations to other objects, together with which, it is now supposed, it constitutes a whole. The necessity of the use of this synoptic procedure in order to give an account of the part is a criterion of the partiality of the objects it investigates, and this implicates the objective existence of a real whole independent of my chance predications.

The supposition on which the synoptic method is based is that being a part of a real whole makes an empirically determinable difference to the characteristics of some parts. We may give a symbolic characterization of a whole as follows:



We may consider b and b' to be distinct wholes consisting of parts and exhibiting the inhomogeneities necessary for our asserting their integral existence. c and c' are parts of b, and b is a part of a. Now it is the ideal of scientific methodology to carry on analysis until a limit of supposed simplicity is reached; this is the

atomistic aim of science. It is just as true, though, that in any specific investigation we are interested in analyzing only to the next lower level of complexity. The complete analysis of a chemical compound would be into electrons and protons, but we are generally content to stop analyzing when we reach recognizable ions whose analysis has already been performed. If a is a real whole and b is a real whole, our knowledge of b is sufficient for most purposes if our synopsis leads us to a and our analysis to c and c'.

Now it is my contention that the only legitimate test of the reality of a as an objective and empirically determinable whole is to be found in certain characteristics of the b-level which are discovered in a synoptic consideration. Why this is so can best be seen in Mr. Spaulding's "A Defense of Analysis."[7] He insists that the products of analysis must be independent of the process of their discovery if the allegation that analysis "falsifies" is not to be justified. But Mr. Spaulding admits that an object may show properties when it is in a whole which it does not have when it is not a part. It is just this fact which is our criterion of real wholeness, but it is a fact which is available only to a method which is synoptic. Mr. Spaulding, while admitting the difference to exist, can not claim that it is a discovery of analysis, for analysis is not to affect the things it discovers. With a strictly analytic procedure, we can not make a comparison between independent elements and parts to see that an object has a property when a part which it does not have when it is not a part, else at least one analysis would "falsify." If we know, as Mr. Spaulding knows, that the properties are not always the same in the two cases, we are prevented, if we make a subsequent analysis of the part, from attributing its entire nature when a part to *its* parts and their intrinsic relations. But if we can not suppose this subsequent analysis to begin with this same part and treat it as a whole by investigating its parts, obviously analysis is not an exhaustive investigation of the part. To return to our diagram: if we analyze a into b and b', and then begin an analysis of b which will then be regarded as a whole instead of as a part, and find that b in the two cases is not empirically the same, then we can know that a is an objective whole since it makes an empirical difference to b when it contains it as a part. But this discovery of difference results from our use of a method which forces us to regard the object b as essentially a part. And this is just what is meant by synopsis. This dual direction of interpretation of an object, by reference to both the next higher

[7] E. G. Spaulding, "A Defense of Analysis," *The New Realism*, New York, 1912.

and the next lower level of complexity, gives us occasion to determine to what extent its properties are intrinsic to it as oversummative to its parts, and whether or not there is some more inclusive whole which has an effect on it. A categorial framework for interpretations of experimental results (e.g., of regeneration) may be worked out with reference to these structural relations.

So considered, synopsis seems to me to be an unavoidable aspect of scientific method, though many who have held to the narrow elementaristic presuppositions of analysis have not appreciated the great significance of the statement that an empirical analysis may change the properties of the parts. In fact, by so considering synoptic method many of the ambiguities in appeals to the whole as ground of explanation are avoided in that the concept is given an empirical, even an operational, meaning. No doubt the careless appeal to the "whole" as the explanatory ground for all mysteries might at times provide an obstacle to investigation: it need not do so, however, if one recognizes that an appeal to the whole means no more a surrender of empirical method than an appeal to the parts does. For this reason, I can not think Dr. Needham's terms such as "obstructive organicism" need frighten us all into accepting his organicism which admits of no discontinuities.[8]

The concept of whole can thus be brought into scientific categorization with as much legitimacy as that of cause, and canons for its application can be formulated. And philosophy will have to learn the same lesson with regard to it as it learned with reference to causality: what is a real whole and what is a cause are matters for empirical investigation, and no amount of ratiocination alone will answer these questions. This is not to deny, of course, that both cause and whole have *a priori* aspects which must be investigated, in one case leading to the concept of ground and in the other to that of system. These two concepts in their *a priori* aspects have an important rôle to play in the synthesis of scientific knowledge, but their application extends far beyond the natural sciences. The concept of whole may well be the central category for philosophy, guiding us in a synthesis of our knowledge and showing the relevance of the determination of the parts by the whole to the general problems of metaphysics.

LEWIS W. BECK.

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[8] Joseph Needham, *Order and Life*, New Haven, 1936.

ON THE EXISTENCE OF AN EXTERNAL WORLD

THE discussion of the existence of an external world in Reichenbach's latest book, *Experience and Prediction,* has set several disputes going in the camp of logical empiricism. One sign of the disturbance we already have in Mr. Ernest Nagel's sharp review of the book.[1] It is doubtful too whether other empiricists will welcome with any more joy this new addition to their list of theoretically legitimate problems, even though that list has expanded considerably since the publication of Carnap's *Logical Syntax* to include now problems of semantics and pragmatics. For one thing, Carnap had already rejected the problem in 1928; and, besides this, in the development from his *Der Logische Aufbau der Welt* to his present position, one of the most striking changes has been his substitution of a physicalistic language as basic for science for the perception language of older positivism, and the acceptance of the former as a base does in itself seem to imply an initial unwillingness to accept the problem of an external world. Mr. Nagel's criticism of Reichenbach on this question repeats again the situation—his own words seem to show he is aware of this—following upon the publication of Bertrand Russell's *Our Knowledge of the External World,* when John Dewey denied that Russell had actually succeeded in stating in that book a problem of the existence of an external world. There is considerable significance in this repetition, since the problem seems to be one which haunts empiricism. Men like Mach, Avenarius, and J. S. Mill, are usually accused (by realists, as, e.g., M. R. Cohen) of being sensationalists and of attempting to reduce the world to a conglomerate of sensations, even despite the pains Mach took to make clear that he rejected the problem of an external world, and so, of course, subjective idealism! It was this taint of subjectivism spoiling positivism's past against which Schlick objected in his later essays (see, for example, his "Positivismus und Realismus") when he indicated that the logical empiricist would also be an empirical realist. So because Reichenbach's new exposition of this problem raises so much of positivism's past, it is worth while pausing to examine it, and to take stock of the general situation here for empiricism. In this paper, accordingly, I would like to remark upon two problems: (1) where Reichenbach's discussion leaves the problem of the existence of an external world; and (2) whether—independently of Reichenbach's analysis—a legitimate problem can be raised here.

[1] This JOURNAL, Vol. XXXV (1938), pp. 270–272.

I

The problem of the existence of an external world, as Russell earlier pointed out, can take many forms. And it is very easy to see that many of these do not have sense. Thus it is necessary at the very outset to indicate the precise form in which Reichenbach attempts to ask this question. The inference to an external world takes the form here of an inference from impressions to the existence of corresponding physical objects, which Reichenbach calls concreta. There are two philosophical problems concerned with existence, Reichenbach indicates: the question of the existence of abstracta (universals) as distinct from concreta (particulars), and the question of the existence of concreta in relation to impressions (p. 93). The lines along which Reichenbach answers this question will perhaps serve to define a little further the sense in which it is asked. He concludes that we *can* make the inference—a probability inference—from impressions to the existence of corresponding external objects. Indeed, the whole problem becomes for him but another illustration of the dominance of the concept of probability in all philosophic problems: it is only when our thinking is obsessed with the ordinary two-valued form of logic that the inference from impressions to external objects becomes meaningless.

The problem and its resolution, briefly sketched above, Reichenbach expounds through a rather elaborate image of the inhabitants of a world who seek to infer to the existence of objects beyond that world. Let us imagine, he says, a cubical world so constructed that the human inhabitants of this cube can not penetrate beyond its walls, these walls, in turn, being translucent so that they leave the shadows cast from objects existing outside the cube visible to the inhabitants within. Birds flying outside the cube cast shadows upon the walls, and the phenomena of these moving shadows upon the walls can be interpreted by the people within according to two hypotheses. The positivistic hypothesis would simply construct a formal law which would seek to state comprehensively how the shadows upon the walls vary in relation to each other. A realistic hypothesis would assert that there are birds outside the cube, whose movements produce the moving shadows on the walls. Reichenbach argues that these two hypotheses do not have the same content, and so long as there is a surplus of meaning (and not simply of statement) in the realistic hypothesis, the problem of the existence of an external world can be stated as a meaningful problem. Now both hypotheses, according to Reichenbach, have exactly the same predictive value for all future facts observed within the cube—but the amazing fact is that the two theories obtain from the facts observed within the cube, different weights! (Reichenbach's dis-

tinction between weight and probability is not of importance here and so need not concern us.) Obviously, two equipollent theories (I use now the more exact expressions of Carnap) must always have the same degree of confirmation, since the facts that confirm the one will always confirm the other, and to the same extent. And the realistic and positivistic hypotheses can not be equipollent if they obtain different degrees of confirmation. The hub of Reichenbach's whole exposition of the problem of the existence of an external world as a meaningful problem consists, then, in the proof that these two hypotheses have, in his sense, different probabilities.

At precisely this point too, we think, the reasoning of the book becomes vague. There is given us only a brief note (p. 124) to the effect that the two hypotheses acquire different weights by the application of Bayes' rule, and that this difference arises from the different "initial probabilities" stipulated in that rule. It is very difficult, however, to see how the rule of Bayes is relevant to that problem of the existence of the external world which Reichenbach claims to be analyzing. The assignment of a weight or probability to any proposition is, as Reichenbach, too, would insist, an empirical procedure: the weight or probability assigned is dependent upon empirical evidence. On the basis of what empirical evidence are the initial probabilities involved in the application of Bayes' rule to be evaluated by the inhabitants of the cube? Only by verifying in miniature systems constructed within the cube (whether they be artificially constructed or natural) wherever similar phenomena of correlated shadows occur that there are externally existent bodies which produce these shadows.

This last procedure involved in applying Bayes' rule to the situation for the inhabitants of the cubical world is just that part, I think, which invalidates Reichenbach's argument. For the problem which his elaborate example, it so turns out, illustrates is not of the existence of an external world at all. His example does not touch upon the inference from impressions to an external world, but upon the inference from a partial to a more inclusive context of physical objects, or *from the properties of certain existent physical objects that are observed to the existence of physical objects that are not observed.* Thus Reichenbach has not successfully carried through the exposition of the problem of the existence of an external world as he originally states that problem, and he has not, therefore, demonstrated the meaningfulness of such a problem.

Besides the invalidity of this main procedure, there are certain other disconcerting, and even confusing, features of Reichenbach's analysis, which we may refer to briefly before passing on to some final remarks on the possibility of the problem in general. One

puzzling thing is that many of Reichenbach's conclusions are in agreement with the physicalistic turn which empiricism has now taken: for example, his insistence that we do not perceive impressions directly, but that impressions are abstracta to be understood in terms of the interaction of physical bodies; his denial that there is a distinct realm of "psychical experiences" which we observe in introspection; his conclusion (the one positive conclusion, we believe, to his lengthy discussion of an external world) that there is no equivalence between statements about impressions and statements about physical objects, a result which Carnap had previously formulated,[2] and more precisely; and, finally, in his epistemological construction of the world upon the basis of concreta. Now one would not expect it as likely that acceptance of the physicalistic basis for the language of science would lead us to raise the problem of an external world, which belongs to that quite distinct phase of positivism, in which it attempted to reduce knowledge to an absolutely certain basis in impressions and atomic facts. Certainly, if Reichenbach had already considered the subjects treated in Chapter III of his book, *An Inquiry Concerning Impressions,* (where he shows impressions to be abstracta from physical bodies), he could not have engaged in Chapter II in the long exposition of the problem of an external world. Reichenbach seems to be aware of a certain dubious character in his procedure, when he remarks that "It is only the consequences of this presupposition (viz., that we have experience of impressions distinct from physical objects), not its validity, in itself, which we want to study for the present." But why has he followed this procedure, so painfully at cross purposes with itself? This last remark quoted seems to imply that Reichenbach doubts the meaningfulness of the problem he has been at such great lengths to expound. Certainly, one can be sure that probability is a most important concept for science and philosophy without going to the length of manufacturing dubious problems where that concept is to be called into play.

II

Can, then, a legitimate problem be raised concerning the existence of an external world? In this section I hope to indicate why I do not think that the problem may be meaningfully raised. If the question is to be asked, it must be asked in a language, and we are naturally led to inquire in which of the various scientific languages it is to be a possible question. (Reichenbach at one point, be it noted, attempts to approach the problem from this point of

[2] "Testability and Meaning," *Philosophy of Science,* Vol. 3 (1936), pp. 465–468.

view when he discusses briefly [pp. 135–145] the contrast between an egocentric language and the usual language of every-day life. His construction of the egocentric language, however, is left very incomplete, and thus nothing very much comes of the contrast.) We will consider the two possible languages for science which are constructed respectively (1) from a basis of physicalistic terms, and (2) from a basis of perception terms,[3] which we will call the physicalistic and perception languages. Now the surprising thing is that the problem of the existence of an external world is much more easily raised (at first glance, at any rate) in the physicalistic language. The question "Does an external world exist?" is to be translated into the question "Does a physical object exist?" These are readily seen to be equivalent since if a negative answer is given to one, a negative answer will also be given to the other. (Moreover, this form of the question is practically that in which it is raised by Descartes.) Is this latter question acceptable for a physicalistic language? I think we may easily see that "physical object" is an observable predicate in some cases, in other cases a testable predicate, certainly in all cases a confirmable predicate. We send a subject into an absolutely bare room, and he returns to report that there is no physical object in the room; if, again, a table is placed in the room, the report becomes that there is *a* physical object in the room. "Physical object" is thus an acceptable predicate for a physicalistic language. The form of our question about the existence of an external world would then run:

(1) $(\exists x,y)$ Physical Object (x,y)

We take "x" as the abbreviation of the spatial coördinates, "y" as the designation of a time. The question of the existence of an external world then becomes "Is there a space-time point at which we can locate a physical object?" (1) is then certainly an acceptable sentence for a physicalistic language provided we are willing to accept unrestricted existential operators in our language, an acceptance which has been criticized by some empiricists, as by Popper and others, and the present instance of the possibility of raising the question of the existence of an external world on the acceptance of such unrestricted operators may perhaps even be urged as an argument against their acceptability. But if our language is to conform to scientific practice, then we ought to accept these operators, since their rejection would greatly impoverish the expressive power of science.

[3] For the general method of this construction, as well as for a statement of the nature of the physicalistic and perception bases, see R. Carnap, "Testability and Meaning," *Philosophy of Science*, Vol. 3 (1936), pp. 419–471, and Vol. 4 (1937), pp. 1–40.

The question thus appears at first glance to be acceptable according to all canons of empiricism. On second glance, however, it turns out that it is not. For to answer the question, to determine any sentence of the form "Physical Object (a,b)"—where the constants "a" and "b" together designate one particular space-time point—always involves the assumption of the existence of other physical objects, for example, in the illustration we chose, the room into which the subject goes. And so generally we can determine the existence of a physical object or objects only with reference to some existing context of physical objects. How does this fact come about? And how is it that it does not appear in the form of (1)? Actually, it does appear in our language, not perhaps from the point of view of its syntax or even semantics, but from the point of view of the pragmatics of the language. Thus pragmatic analysis of our language tells us that actually in the experimental procedure of science symbols like "a" and "b" are not proper names but *descriptions*. And also for the individual variables of that language the range of substitutible expressions is a non-denumerable aggregate, not of proper names, but of *descriptions*, of space-time points. And in each of these descriptions there will always be some component reference to physical objects. Thus the space-point designated by the proper name "a" is (in the actual procedure of science) to be designated by some such description as, say, "the point which would be reached by a certain number of applications of a yardstick beginning from such and such a point on an object." Generally, that is, all space-time points have to be ascertained by reference to some material body. The truth or falsity of (1) would become determinable only if one presupposes the existence of certain physical bodies. (1) is to be eliminated through the application of a vicious-circle principle.

We have now to go over to the perception language (the scientific language of Mach and of Russell) and see whether the problem of the existence of an external world can be acceptably stated there. As we have said, it is more difficult to state the problem thus in any precise form—the question of final acceptability aside—than for the physicalistic language. In the first place, it is clear that such a predicate as "physical object"—and indeed physicalistic predicates generally—can not be a basic predicate. We have authority for this as far back as Hume, who insisted that there was no distinct impression—and so no corresponding distinct idea—of external existence, but that this idea of existence was but an idea of coherence among our impressions; and he gives the illustration of the traveller in the desert who sees from the hilltop the mirage of a pool below him, which, when he arrives below, yields no tactile impression of water.

Hume's general point is to be accepted here, with some provision made for more exact statement. A predicate like "physical object" is to be introduced into the language by means of reduction sentences[4] (not explicit definitions). The individual variables here designate individual experiences of perception, and the sentences of this language run like "*a* is a visual impression of water," "*b* is a tactile impression of water"; and the problem of the physical existence of the water amounts for the desert-traveller logically to this: If *a* is a visual perception of water, is there then an *x* such that *x* is the tactile perception of water (to be located at the same perspective place as the visually perceived water)? This is, of course, very rough, and many more qualifications need to be introduced; but it serves to point the way in which reduction would take place. That reduction need not be carried through here; to testify to the fact that it can be done one need only point to Carnap's *Logische Aufbau der Welt,* where a much more radical and thoroughgoing reduction is achieved.

The foregoing considerations serve to indicate what was said earlier, that the decision on the acceptability of the question of the external existence of a world can not be settled with the same formal simplicity for a perception language as for a physicalistic language. To make the decision we shall invoke what we may call the principle of parallelism between the physicalistic and perception languages. This principle means—to put it in its simplest, most pragmatic terms—that the physicist who adopts a physicalistic language and the physicist who adopts a perception language have to perform the same experiments to discover a given fact. Thus a physicist like Mach and another physicist using a physicalistic language can work together in the laboratory: they have, in fact, to perform the same experiments. There is nothing in experimental or theoretical physics which Mach could achieve that a physicalistic physicist equally precise in his methods could not also achieve. At first sight, this objective community of fact, so to speak, between the two physicists might seem like a refutation of Carnap's thesis that the choice of a language is conventional; but, in fact, it only supports it. For, expressed from the point of view of the analysis of language, our statement above asserts: for every expression, or set of expressions, in a physicalistic language there will exist a corresponding translation in the perception language, and there being thus no essential difference in expressiveness between the two languages, the choice between them is a matter of convention.

If we accept the parallelism between the physicalistic and perception languages, then our results in the first case can be applied

[4] For the technique, see Carnap, *loc. cit.*

now to our present problem. As we determine whether a physical object exists only by presupposing the existence of other physical objects, so here we decide whether certain impressions cohere (using still the expression of Hume) only within a framework of impressions already accepted as coherent. Our traveller in the desert can test whether a tactile impression will cohere with a visual impression of the water only within some framework of impressions accepted as coherent—as, for example, his impressions that his sense of touch has not been lost, that he is not under hypnosis, that *the sand he has apparently traversed in order to reach the water is physically existent, and not illusory*—and the rest. To express this more exactly: assuming that the term "physical object" has been suitably introduced, we may put the question of the existence of an external world for a perception language in the following stricter form: (2) For every sentence of the form "I have now an impression of a physical object" or "Here now there is an impression of a physical object" (to adopt in the latter a more thoroughgoing phenomenological statement) will there exist some well-confirmed report-sentence of an impression which will deny some logical consequence of the sentence "I have now an impression of a physical object"? A negative answer to this question would affirm the existence of an external world. But (2) is open to the same objection as was seen previously. How is the report-sentence referred to in (2) to be given a degree of confirmation unless it is tested within a frame of impressions which is taken as cohering?

The above results hold for a perception language which is phenomenological; like results would hold, of course, for a physicalistic language which confined itself to the choice of perception predicates alone as basic. In neither of the languages so far considered can the problem of the existence of an external world be stated as a genuine problem.

One or two remarks may be made in conclusion. If we consider the practical reason for raising a problem—even where "practical" is taken in a quite inclusive sense as designating anything serviceable for our understanding—there seems no ground at all for the problem of an external world. So great, in fact, is the gap between this problem and any practice that one must always stand convicted of shame when one tries to communicate this as a serious cognitive problem to anyone innocent of philosophy. On this point Dewey's rejection of the problem is likely to prove reliable.[5] No doubt the problem of an external world as it was given

[5] We agree with Dewey that Russell does not succeed in raising a genuine problem in Chapter III of *Our Knowledge of the External World.* Russell rephrases the problem to read: "Is there any inference possible from our

to modern philosophy by Descartes was generated out of a practical situation: the bewilderment in his own beliefs occasioned by the rapid changes in scientific knowledge at his time. The problem of the existence of an external world arose in the process by which Descartes sought readjustment among these beliefs. But it was from the start a false problem. Descartes asks us to imagine the possibility that some devil is producing these appearances which we call a physical world. This we may easily imagine, but even if the devil were producing them, these appearances would still retain the character of being a physical world. There is a difference involved, no doubt, since the devil may by a sudden arbitrary caprice abrogate any law of nature or perhaps even nature itself. Descartes' statement thus conceals his real question, which in fact concerns the constancy of the laws of nature—a problem similar to Hume's problem of induction, but certainly not the problem of the existence of an external world. Or, as is probable, the question was involved for Descartes with religious feeling (there is some probability to this because we do find that his systematic doubt is controlled by the religious beliefs of his time), and Descartes was really asking, "Is the natural world the work of the devil?" If this be the case, the problem reveals itself to have been from the very beginning theological.

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BOOK REVIEWS

La pensée au moyen âge. PAUL VIGNAUX. Paris: Armand Colin. 1938. 206 pp. 15 fr.

M. Gilson and his students have, in recent years, introduced many changes into the study of medieval philosophy, among which two have had a general influence on the interpretation of the period: the notion of a medieval humanism and the consideration of the influence of Avicenna, shown in the doctrines of some Augustinians and combatted by Thomas Aquinas, on the doctrinal oppositions of the thirteenth century. M. Vignaux, a former student of Gilson, has organized his brief account of medieval thought about these notions. In directing his attention to medieval "thought" M. Vignaux has sought to include with the account of

hard to our soft data?" but then succeeds in applying this question validly to the existence of other minds while he lets the question of the existence of an external world slip away. The problem, phrased in his manner, would allow us only to ask about the existence of particular physical objects—as above in the text.

philosophic developments some indication of the place of studies of grammar and logic and of the study of theology during the Middle Ages. His book proceeds in rapid steps: after a brief introductory chapter which brings him to the twelfth century he states the doctrines of four "founders," Anselm, Abailard, Bernard, and Richard of Saint Victory, deals with the "conditions" of the thirteenth century (primarily the University of Paris and the translations from Greek, Hebrew, and Arabic), treats the "diversity of the thirteenth century" in terms of Grosseteste, Roger Bacon, Bonaventura, and Thomas Aquinas, disposes of the fourteenth century by showing two "modes of thought," those of Duns Scotus and Ockham, and finally sketches "aspects" of the fourteenth and fifteenth centuries. His work has important merits: it is the first popularization of interpretations that the reader could heretofore find only in essays and monographs written during the last ten years; it brings significant additions from the works of authors (particularly Duns Scotus) who have been treated by historian after historian in terms of the same problems and by means of the same texts. It is, on the other hand, brief and sketchy, and apt therefore to be intelligible in detail only to the reader who knows the doctrines and the controversies from which the interpretation arose; since it is equipped with only the meagerest of summary bibliographies and is completely without references to guide the reader to the original of its citations and paraphrases, there is no easy means for the unversed reader to test the accuracy of M. Vignaux's statements or to find further information concerning the grounds of his interpretation.

R. McK.

Aufgabe und Entwicklung im Menschenbild des Jungen Nietzsche. HUBERT TELLENBACH. (Kulturphilosophische, philosophiegeschichtliche und erziehungswissenschaftliche Studien. Heft 5.) Würzburg: Konrad Triltsch. 1938. 54 pp. 2.50 M.

The story of the development of Young Nietzsche's thought and personality has often been told. The overcoming of Schopenhauer's pessimism, the fascination exerted by Wagner, the Parsifal incident, the break with Wagner, for example, have been repeatedly explained, and the present version adds very little. Unfortunately, Nietzsche is explained only in relation to Greek philosophy, literary movements, art criticism, and great personalities. No attempt is made to show his real stature against the background of world economic rivalries and social change. It is regrettable too that Frau Foerster Nietzsche is taken so literally.

V. J. McG.

Toward a Dimensional Realism. CHARLES M. PERRY. Norman, Oklahoma: University of Oklahoma Press. 1939. viii + 180 pp. $2.50.

The general claim, from the point of view of which Professor Perry considers many problems in turn, is that reality is much more complex than any analysis can alone indicate, that various and even contradictory analyses may alike be justified from their respective lines of attack, and that the erroneous element in most philosophical systems is the supposition that the system defines the only or the basic approach which all must then continue to follow. This general claim is applied in a series of chapters to many fields, from space-time to society. The use of the term "dimensional," however, is not always helpful: it seems at times to become a solution rather than to serve in the exposition of a solution. Perhaps the insistence that certain dimensions are orthogonally isolated from each other and the explanation that some dimensions may be projected upon each other when they fall within the same quadrant are but luxurious developments of a favored metaphor: perhaps they mean that certain considerations are wholly irrelevant to each other and that some considerations have limited pertinence to each other. To allow a metaphor to become the controlling feature of a book is not always clarifying. But Professor Perry makes many suggestive comments upon recent and contemporary philosophy. And the metaphor of dimensionality lends itself excellently to the contention of the final chapter, namely, that society should foster variety of types and interests within its own rich and "multidimensional" cohesiveness.

S. P. L.

Semiotik. Eine Theorie der Zeichengestalten als Grundlage für Untersuchungen von formalisierten Sprachen. HANS HERMES. (Forschungen zur Logik und zur Grundlegung der exakten Wissenschaften, Neue Folge, Heft 5.) Leipzig: S. Hirzel. 1938. 22 pp. 1.20 M.

The object of this study is to specify formally a set of operations sufficient for constructing a "language" (that is, sets of linearly ordered series of signs) and therefore also sufficient for defining all (syntactical) terms which refer only to the order and transformation of sets of signs. This is achieved by setting up abstract postulates with one uninterpreted non-logical predicate, and which are satisfied by interpreting this predicate as the operation of adjoining one elementary sign to a series of signs. The author shows that within this system it is possible to define all terms and to deduce all

propositions belonging to the theory of sign-structures (syntax), and he carries through this construction in detail for two fundamental syntactical terms—"juxtaposition of signs" and "substitution of signs." He also proves that his postulates have the structure of a generalized arithmetic, and briefly indicates the connection of his system with allied researches by Tarski and Quine.

E. N.

Swords and Symbols. The Technique of Sovereignty. JAMES MARSHALL. New York and London: Oxford University Press. 1939. 168 pp.

This little volume succeeds in doing what Bertrand Russell's more pretentious work has failed to do: it describes the political state as a process, as a continual shifting of points of tension and of exercise of power by the ruling class. This more or less conspicuous changing of ruling classes is due primarily to the fact that "the weapons of sovereignty are divided" (p. 131). An analysis of these weapons and their relative uses is the chief subject-matter of the book. The chief weapons discussed are physical coercion, economic power, law and custom, prestige ("reputation for power"), and propaganda. This last weapon, Mr. Marshall points out, is particularly subtle and powerful and deserves careful analysis, for it is all too easy to confuse symbols of swords with symbols of thoughts. As a contribution to such an analysis, Mr. Marshall examines especially the symbols of "popular sovereignty," "free contract," "liberty," "the withering state," and "the ideal of unity." His description of the uses which these concepts serve constitutes an effective critique of Trotsky's theory of revolution as well as of the older theories of periodic tensions and equilibria among powers and interests. Mr. Marshall defends a large ruling class on the ground that "the broader the base and the more common the interests whose needs are protected the less will be the friction and correspondingly the less will be the demands, the struggles and the readjustments of political power" (p. 143). The book is so incisive, realistic, and genuinely learned that it deserves a permanent place among the classics of political wisdom literature from which it draws.

H. W. S.

The Citizen's Choice. ERNEST BARKER. Cambridge: At the University Press. 1937. New York: The Macmillan Company. 1938. ix + 185 pp. $2.00.

Though the lectures collected in this volume were delivered during the years 1934–1937, they are by no means out-of-date. On

the contrary their importance and relevance becomes daily more evident. Mr. Barker's political philosophy here gains a useful reformulation which indicates the general lines along which British idealists are adapting their ideas to contemporary issues. Especially noteworthy are: "The Breakdown of Democracy," "The Prospects for Democracy," and "Foundations of Politics." From the chapter "Philosophy and Politics," the following passage may be quoted as a pervasive theme of Barker's thinking:

> . . . the method of democracy is the method of debate; and I have said, or implied, that the method of philosophy is the same. Does it not follow that the method of democracy is the true philosophic method, and that the true philosopher, far from being fastidious or pedantocratic, will be a democrat? Plato said that it was impossible for a whole people to be philosophic. It may also be said, in the opposite sense, that the only thing which can be philosophic, in matters of government, is the whole people. [P. 119.]

H. W. S.

Theories of Religious Experience, with Special Reference to James, Otto and Bergson. JOHN MORRISON MOORE. New York: Round Table Press, Inc. 1938. xi + 253 pp. $3.00.

Three contrasting modern examples of the appeal to religious experience as the "source and essence" behind "codes of conduct, theology, ritual, and institutional elements" are ably described and analyzed in this study by Professor Moore. His conclusions are largely negative: ". . . the term religious experience has to a considerable degree outlived its usefulness. Its popularity has rested in large measure upon its vagueness and ambiguity. It has carried comforting suggestions of underlying agreements and impregnable certainties, but these are likely to disappear upon examination. . . . The term suggests an unduly individualistic conception of religion and an oversimplified view of its psychological foundations. . . . Religious experience is not any one kind of experience with constant and characteristic marks . . . it is not a distinct form of cognition, but is dependent upon the cultural environment for its sources of insight and upon philosophical criticism for any claim which it makes to cognitive validity." Do not these honest difficulties in reaching a clear definition of religion or religious experience, as well as the ambiguous position of religion in the contemporary university (a secularized, yet partly spiritual institution) raise the question whether both terms have not outlived their usefulness, except as historical distinctions?

H. A. L.

Nations et droit. Le développement du droit international entendu comme développement de l'état. UMBERTO CAMPAGNOLO. Paris: Félix Alcan. 1938. 305 pp. 50 fr.

In the course of this excellent critique of the theories of international law, the author examines especially their philosophical foundations. He makes a searching analysis of Kelsen's reliance on a theory of absolute values as a necessary foundation for "pure right," and suggests a more empirical theory of the moral relations between both states and individuals. Both the idealists and the realists, according to him, have committed the error of identifying the reality of law with the historical development of the science of law. After showing that the defenders of international law fail to establish it, and that those who deny international law fail to establish the absolute sovereignties of the states, in the interest of which they deny international law, M. Campagnolo develops a theory of law which implies an actual "society of nations" and a possible development of its laws.

H. W. S.

L'épithymologie. JEAN URBAN. Paris: Félix Alcan. 1939. 360 pp. 50 fr.

That a good book on the psychology of desire is needed is indisputable. But it is probable that M. Urban's does not fill the need. In the first place, it suffers from the disease common to many books in philosophy, of making distinctions based upon etymologies rather than on facts. In the second place its documentation is surprisingly out of date. In the third place, by the very nature of its aim as a scientific treatise, it deals with the average man; desires are frequently derived by the author from the unusual human being and then transferred to the general run, and at other times they are deliberately built up (as in advertising and propaganda of non-commercial types) and destroyed. This does not mean that M. Urban has failed to write a provocative and stimulating book. Within its limits it is an excellent specimen of "literary psychology" of the early twentieth century. But that is the most that can be said for it.

G. B.

Selbstmord und Erziehung. Eine kulturphilosophische, psychologische und pädagogische Studie. ALOIS GREITHER. Leipzig: Felix Meiner. 1939. 212 pp. 4.80 M.

In the first part the book gives a short survey of man's opinion of suicide at different periods and in different cultures. In this

section the remarks about primitive peoples and their attitude towards suicide seem to me especially interesting. The second part—in my opinion the most valuable—deals with modern sociological, psychiatric, and psychological theories. The author is undoubtedly right in stressing the inadequacies of statistical and sociological classifications and generalizations and in opposing certain psychiatrists who consider every suicide a symptom of mental disease. One would have wished, however, that the author had dealt at greater length with the theories of Emile Durkheim and his disciple, Maurice Halbwachs, more especially with their rather well-founded hypothesis that one of the principal causes of suicide is the spiritual isolation of the individual from society. This theory seems also to explain why ever since ancient times those philosophers who advocated an individualistic ethics recognized the right to suicide. One quarter of the book is an account of the theories of psychoanalysis with regard to suicide, dealing in a very objective and instructive way with the different stages of Freud's opinions and those of his followers. The last part (on prevention of suicide) is rather disappointing. It deals with general principles of moral education of children and in a much too cursory way with the attempts that were made towards prevention of suicide. The author could have increased the practical value of his work if he had investigated the work of anti-suicide bureaus and personal service stations that has been done in different countries. On the whole, however, the book, which also contains a short bibliography of suicide, shows exact research of the subject and can be recommended as throwing much light on the present status of the problem.

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Le problème de la connaissance. Empirisme et rationalisme grecs. F. ENRIQUES and G. DE SANTILLANA. (Histoire de la Pensée Scientifique, IV.) (Actualités Scientifiques et Industrielles, No. 572.) Paris: Hermann & Cie. 1937. 54 pp. 15 fr.

Platon et Aristote. F. ENRIQUES and G. DE SANTILLANA. (Histoire de la Pensée Scientifique, V.) (Actualités Scientifiques et Industrielles, No. 573.) Paris: Hermann & Cie. 1937. 62 pp. 18 fr.

These pamphlets contain Chapters VII–XI in the authors' forthcoming history of scientific thought. They are written in a clear and unpretentious style, often humorous, and with an economy of detail that suggests a comprehensive knowledge of the field. They

deal with the successive contributions to the philosophy and methodology of science by the Sophists of the fifth century B.C. and by Socrates, Democritus, Plato, and Aristotle. The interpretation of the sources is in the best tradition of French and English scholarship and leans heavily on such authorities as Tannery, Milhaud, Robin, Grote, Burnet, and Beare. Towards the German scholars, with the exception of T. Gomperz, the authors are perhaps inclined to be somewhat too critical, underestimating the importance of the more systematic and speculative aspects of Greek thought in favor of its analytical tendencies. It is only fair to say, however, that these chapters are not directly concerned with the metaphysical speculation of the Greeks, except in its bearing on the development of scientific thought.

The clue to this development is found in the familiar contrast between rationalism and empiricism. The reaction of the Sophists against the rigorous dialectic of Parmenides is compared to the modern positivistic reaction against metaphysics. But the authors are careful not to press the analogy too far. The Sophists had "the same philosophical mentality" as the modern positivists, but their methods differ. "While the moderns would operate only with the support of facts and profess a holy horror for the subtleties of dialectic, the ancients fought with the same weapons as their principal adversary, the school of Elea." Consequently, the logical and linguistic analyses, which the older generation of Sophists had pursued in the interests of positive, empirical knowledge, degenerated with their successors into the logomachy from which "sophistry" was to acquire its unsavory reputation. Socrates represented a return to rationalism in opposition to the Sophists. He sought to justify the claims of "pure intelligence" by turning the sophistical methods of analysis against their own relativistic and sceptical conclusions. But it was Democritus and Plato who brought this reaction against empiricism to its completion. They gave precise signification to the distinction between the "intelligible" and the "sensible," and each in his own way tried to satisfy the requirement which the Sophists had imposed on every rational construction of science, namely, that it must "save the appearances." The definition of science in the *Theaetetus*, as "true opinion accompanied by reason," probably originated with Democritus, and it "almost agrees with the 'experimental rationalism' that modern thought has elaborated since Galileo. . . . The way is thus paved for the doctrine of Aristotle: ideas are derived from sensations, but have much more validity than these because their contents are the veritable models of things; science finds its measure in the common sensible and not in subjective sensation."

In contrast to the "experimental rationalism" of Democritus, Plato's is described as a "formal rationalism," in which moral and mathematical analogies play the major rôle. His positive contributions to scientific thought were slight and his interest in it seems to have come only from "the motives of art, religion, or morality that were attached to it." But in his essentially dramatic and analogical arguments, there is the germ of a principle that regulates all scientific research: the principle of the simplicity of nature. Aristotle tried to bring this Platonic insight into accord with empirical reality by means of his vast "encyclopedia of knowledge," but the attempt was not wholly successful because of his failure to understand the significance of mathematical physics in Democritus.

D. S. M.

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THE AUSTRALASIAN JOURNAL OF PSYCHOLOGY AND PHILOSOPHY. Vol. XVII, No. 1. A Classical Approach to Mathematical Logic: *T. Greenwood*. Mystical Experience and Philosophy: *Greta Hort*. The Development of the Conception of Mind: *D. Taylor*. Discussions—Some Implications of Morality: *R. A. Miller*. Logic and Ethics: *John Anderson*.

REVUE INTERNATIONALE DE PHILOSOPHIE. Première année, No. 3. (Le Réalisme Anglo-Saxon.) American Philosophy in the First Decade of the Twentieth Century: *R. B. Perry*. L'histoire du réalisme américain: *Wm. P. Montague*. A Statement of Critical Realism: *R. W. Sellars*. L'esprit du néo-réalisme anglais: *Philippe Devaux*.

GIORNALE CRITICO DELLA FILOSOFIA ITALIANA. Anno XX, Fasc. II. La psicologia d'Epicuro e la teoria delle passioni: *Carlo Diano*. La filosofia degli idealisti cattolicizzanti (cont.): *Gaetano Durante*.

DIE TATWELT. 15. Jahrgang, Heft 1. Dem Jahrgang 1939 zum Geleit. Die geschichtsbildende Kraft der Lebensanschauungen grosser Soldaten: IV. Graf Schlieffen: *Friedrich von Boetticher.* Wissenschaft und Lebenspraxis in der Gegenwart (2. Bericht): Referat: *Wolfgang H. Veil.* Tatsache und Schicksal: *W. E. Hocking.*

NOTES AND NEWS

We regret to learn of the death of the Reverend George D. Bull, S.J., Professor and Head of the Department of Philosophy at Fordham University Graduate School, on May 27th, 1939.

We have received the following communication entitled "Aims and Scope of the Journal of the History of Ideas, A Quarterly Devoted to Intellectual History." It is hoped that the first issue will appear in January, 1940.

"It is believed by a number of American scholars in several fields of historical study—the history of philosophy, of literature and the arts, of the natural and social sciences, of religion, and of political and social movements—that there is now need of a journal which will emphasize the inter-relations of such studies and afford, especially, a medium for the publication of researches which cross the customary boundary-lines or are likely to be of common interest to students in different fields. Much valuable work of this type is now being done, but it is widely scattered through different specialistic publications; and adequate vehicles of reasonably prompt publication are lacking. The proposed journal would aim, so far as its resources permit, to bring together many of these unrelated studies and to promote greater collaboration among scholars in all the provinces of intellectual history in the West. There exists no such journal in the English-speaking world.

"As concrete illustrations of topics to the illumination of which such a journal may contribute, the following may be mentioned:

1. The influence of classical on modern thought, and of European traditions and writings on American literature, arts, philosophy, and social movements.
2. The influence of philosophical ideas in literature, the arts, religion, and social thought, including the impact of pervasive general conceptions upon standards of taste and morality and educational theories and methods.

3. The influence of scientific discoveries and theories in the same provinces of thought, and in philosophy; the cultural effects of the applications of science.
4. The history of the development and the effects of individual pervasive and widely ramifying ideas or doctrines, such as evolution, progress, primitivism, diverse theories of human motivation and appraisals of human nature, mechanismic and organismic conceptions of nature and society, metaphysical and historical determinism and indeterminism, individualism and collectivism, nationalism and racialism.
5. Neglected but historically significant writings and ideas of minor as well as major figures in the history of thought.
6. The relative roles of logical and alogical, or non-intellectual, factors in the shaping of doctrines, beliefs, and 'climates of opinion'; the sociology of knowledge.

"Advance subscriptions will expedite the issuance of the journal. Each number of the quarterly will contain 128 pages of articles, book-reviews, etc. Subscription rates (domestic): $4 per annum, $7 for 2 years, $10 for 3 years, $1.25 for single copy, (abroad) $4.50 per annum, $1.40 for single copy. Please make checks payable to *Journal of the History of Ideas,* Inc. (College of the City of New York, Convent Ave. and 139th St., N. Y. C.).

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